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A

NEW THEORY

OF

YELLOW FEVER,

FOUNDED ON THE RESULTS OF

CHEMICAL EXPERIMENT.

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PRELIMINARY REMARKS.

It is generally admitted, that in all investigations into subjects, whose conditions are complicated, too much precision cannot be exercised in prescribing certain rules of ratiocination by which we should be governed in our results. The following, therefore, will claim the particular attention of the reader in the perusal of this essay. 1. We should never recur to invisible agents or gratuitous principles to explain effects, while such as are obvious, and more intelligible, can be demonstrated to be adequate. 2. In explaining the operation of a power to which an effect is referable, we should never assume principles that have not been proven to be true 3. Where several powers can be shown to co-operate in the production of an effect, we are bound to include the whole, as constituting the true cause of such effect. 4. If an explanation be shown to be founded upon principles, directly contradicted by observation and experiment, the theory again returns to its original character of a gratuitous position. The importance of these rules will be highly evident, on a superficial view, particularly as relating to the doctrines of electricity, contagion, and miasmata; these being the only ones whose error I have deemed it necessary to expose. Indeed, much of their futility, it will be perceived, consists in an open violation of some one, or all of those maxims of legitimate argumentation. The tendency of such violation, must place us in the predicament of advancing opinions devoid of proof, direct or presumptive; of taking such a partial view of a subject, as to assign but a part of the real cause; of supposing an effect to be produced by a power, which, from its nature, can act but a negative part; of being led to conclusions that have but a negative connection with the data assumed; or, of establishing inferences which are foreign to, or unwarranted by the premises from which they are drawn. In addition to this, there is another view which may be taken, of the three hypotheses above alluded to, equally indicative of a common error. I have enumerated heat, moisture, putrefaction, vegetation and respiration, as the principal powers concerned in the production of yellow fever; or, at all events, I have demonstrated that they all absolutely co-operate in bringing about a condition of the at

mosphere, which is confessedly, prejudicial to health. Now, the authors and advocates of electricity and contagion reject the whole of those *known* powers, and adopt principles shown to be gratuitous; while those who refer the disease to miasmata from putrefaction, reject all the rest. But common sense tells us, that to produce disease, it is only necessary to disturb those agents upon which health depends. The principal one, in the present case, is atmospherical oxygen, whose ordinary quantity has been shown to be diminished by all of the circumstances enumerated, except electricity, contagion and miasmata. Admitting, however, that any one of those circumstances gives rise to yellow fever, it would evidently follow that there are two ways of effecting it, viz. by a *part* of the real cause, which is efficient; or, by *the whole* taken in conjunction, which, nevertheless, lies idle—which is absurd. Should my own notions be found erroneous, I shall cheerfully relinquish them: I trust, however, that what I have endeavored to substantiate, will not be deemed unworthy of farther consideration and research.

NEW THEORY, &c.

CHAPTER I.

THE opinion advanced in this essay maintains, that the cause of yellow fever consists in such an enlargement of the general atmospheric mass, relatively to the quantity of oxygen which it contains in a given bulk, as to preclude the requisite quantity of that vital portion from entering the lungs at each inspiration. The powers which are supposed to give rise to this enlargement, are heat, moisture, miasmata, the vegetation of plants, the respiration of animals, and every other circumstance in nature, capable of such an action. The grounds and reasonings, by which this opinion is thought to be legitimately substantiated, are to be found in the following pages:

Ground 1. It is now generally admitted, that the healthy energies of all vegetables, as well as animals, throughout both kingdoms of animated nature, absolutely require a due and uninterrupted supply of oxygen gas for the performance of their functions: This gas essentially constitutes one-fifth of the bulk of the atmosphere in an uncontaminated state; and as it relates to animals in particular, it is in this combination that it subserves the purposes of vitality best. I believe, to these positions, as relating to those two classes of living functions, there has never been discovered an exception, either in the natural history of their subjects, or in the demonstrations of experimental philosophy. Again, during every living process, as it relates

to atmospheric influence, under a healthy, vegetative action, both by the germination of seeds, and the vegetation of plants, as well as, by the respiration of animals, the oxygenous portion disappears, and a corresponding quantity of carbonic acid is produced in its stead, without in any manner affecting the azotic principle of that fluid. When the due supply of pure air is kept up, the beings whose actions depend upon its agency, are uniformly observed to be energetic, and to perform all their functions with perfect facility and vigor; and in proportion as the vital ingredient is abstracted, they become sickly, enfeebled, and their energies, in every respect impaired, or entirely suspended. These are truths, which are so well established, at the present day, as to render it superfluous to insist longer upon their correctness in this place. Those, who may have any doubts, would do well to consult the latest authorities on chemistry and physiology, and particularly a work by Mr. Ellis on air, together with the authorities to which he refers.

Ground 2. On a minute examination into the history of the disease, I feel warranted in drawing a conclusion that, it never occurs, but at those places where, and during those periods of the year and weather, when every natural cause which is calculated, and necessary to create this enlargement of the atmosphere above-mentioned, is brought into operation in its highest intensity. By thus invariably preceding, or accompanying the disease, I think there can be but little doubt of there being some necessary conneixon between their agency and its existence.

Ground 3. When that period of the year rolls round, or that state of the season occurs, which either by its initial, or consequential impression, suspends the operation of those causes, which alone

can produce a derangement in the proportion which the oxygenous principle ordinarily bears to the whole bulk of the air, the existence of yellow fever, invariably vanishes. Does not this argue, upon the face of the fact, that, if those *causes are not* the direct agents in the production of the disease, there may, at least, be a very close association between them upon some natural principle. By an exposition of the manner, in which these causes are presumed to operate, this connexion, I think, will be amply indicated.

Ground 4. So far as I am able to deduce causes from their effects, and to trace the connexion which physically obtains between them, I feel constrained, after an impartial investigation of facts, and circumstances, and upon every principle of fair argumentation, to conclude, that, in no country, nor during any season of the year, however aggravated a form it may have assumed, has yellow fever ever manifested a contagious tendency. This is the general sentiment of the day; I believe it has been, in a great measure, the opinion of a majority of those who have had most experience in its practice; and it is one, which I conceive, would follow as a natural and necessary consequence, if my first position be correct.

Ground 5. Persons from an elevated part of the country, from high latitudes, and other situations, where the causes above alluded to, cannot be supposed to operate in a high degree, are far more liable to an attack, than natives and long residents of the place where the disease prevails. The relavancy of this ground will be obvious upon a deliberation of the force and influence which habit is known to exert, over the feelings and movements of the animal economy.

I might here detail many other considerations, from which cogent deductions might be drawn in favor of the doctrine contended for; but their bearing not being direct, and to render them sufficiently clear would require a latitude of discussion and detail, not at all anticipated on the present occasion, I shall mention but one more ground of material importance to my purpose.

Ground 6. The total, and generally acknowledged inadequacy of every hypothesis hitherto advanced, either to develope the causes of the disease, or to expose the manner in which they have been supposed to exert their energies in its production. The validity of this latter ground can only be tested by a concise recurrence to some of the leading and most generally received speculations of our schools, at the present day, upon this important topic of medical philosophy, which will be contained in the two following chapters: I am aware that the most authoritative maxims of logic, as well as every principle of correct philosophising generally, require, that in offering new theories for the explanation of a series of phenomina, we should not only demonstrate a perfect reconcilability of our own positions to those phenomena, together with a thorough adequacy to their elucidation; but that, at the same time, we also expose some palpable repugnance to, or incompatibility with, those phenomena in regard to all other opinions. The importance of this double injunction seems not, at all times, to have sufficiently impressed the votaries of our profession; and through the neglect of which, from a fondness for intruding our own notions upon the world, gentlemen have too often suffered their imaginations to supply, what a more dispassionate judgment, and a more labored investigation should

have pointed out or rejected. This would have purged our schools of the multiplicity of speculations and intellectual vagaries, which have too often deluged their departments, and for a time diverted the footsteps of rational inquiry from their proper channel. Having long weighed the force of the above remarks, I will conclude this first chapter, containing a simple enunciation of the grounds upon which my opinion is predicated, by declaring, that I feel perfectly warranted in the belief that there is not a fact or circumstance connected with the prevalence of yellow fever in any quarter of the globe, in any degree repugnant to, or contradictory of, the position I adopt; whilst, of the old notions, some are too vague and irrevalent to deserve serious attention, and the rest fall vastly short of affording any satisfactory illustration of the subject.

CHAPTER II.

Among the great diversity of conjectures which have been thrown out at any period of the world, on the subject of epidemics generally, I find but three which are seriously resorted to at the present day, for the explanation of yellow fever. These are electricity, contagion, and miasmata, from vegetable and animal putrefaction. As it regards therefore, the *To Theon* of the ancients, astral influences, invisible animalculæ floating in the air, a nitro-aerial spirit, corrupted by an aura from the earth, arsenical, bituminous and other mineral effluvia, and a thousand other superstitious follies and absurdities, they may be passed over as being too ridiculous to require any particular refutation; for I presume, that at this enlightened era of our science, their bare mention is sufficient to carry their refutation with it. It serves, however, to display in what total mysticism the subject has, at all times, been enveloped, when we are told that for the illustration of a phenomenon, such a dissonance of opinion has prevailed. It must leave the judgment of the young inquirer in perfect suspense, when he has learned, that with all the erudition and experience of his predecessors, their speculations should be marked by features so diametrically opposite in their nature. I will now proceed to consider, separately, the merits of the three opinions of the day, above mentioned, in as concise a manner as their nature will admit of.

SECTION I.

THE DOCTRINE OF ELECTRICAL INFLUENCES.

So far as I can learn, there have but three attempts ever been made of an express character, to prove the origin of diseases from the agency of the electric fluid. As the views of their authors were somewhat different, I shall consider them, individually, according to the order of time in which their productions were given to the public. The first, therefore, whose notions demand attention, was the Abbe Bertholon of France. Having minutely examined the effect of this agent upon muscular motion, digestion, secretion, &c. in the human body, he arranged all diseases into ten classes; some of which originating from a vitreous, whilst others were dependent upon a resinous electricity. He supposed the seat of these two fluids to be the atmosphere, and that they were absorbed, into the system, through the pores of the skin. Their operation, he contended, consisted in some direct exertion upon the nervous, muscular, and secretory susceptibilities of the several organs, whereby digestion became enfeebled, its proper sympathies deranged, all the natural and vital functions disordered, and thus the foundation of diseases laid. But the insufficiency of such an hypothesis must, I presume, be rendered perceptible by the following considerations: The doctrine is, in itself confessedly assumptive, and is therefore unsupported by any direct experiment. And I am confident, that any observations and reasonings, which he has adduced, either of a direct or analogical nature, have not been founded upon established principles of physiology, nor upon the visible symptoms of diseases. Without which requisites they cannot be brought into competition with demonstrations made to the senses.

From any thing, we at present know of the animal economy, of the phenomena of disease, or of the operation of physical agents, generally upon the living system, I discern no reason, in nature, nor is there any legitimately *assigned*, why the effects in question might not, with equal propriety, be referred to any other power, as to that of electricity. We should expect some unequivocal facts, either experimental or natural, showing this to be its tendency. He should have furnished us with some signs or appearances of the times, whereby we might be informed of the circumstances which favor an accumulation of a vitreous, or of a resinous electricity in the atmosphere. We should have had some exposition of the reason why innumerable maladies prevail at the same place, and at the same time, differing essentially in their symptoms, progress, sensations, treatment and consequences. Nor is there any attempt whatever, made to point out the fallacy, or inadequacy of the causes *usually* assigned. This mode of conducting our speculations, certainly cannot be reconciled, without doing violence to the first principles of enlightened philosophy. Ample proof is exhibited on the face of a thousand familiar observations of the day, together with innumerable facts recorded in the annals of our science, that a sufficient number of obvious, as well as demonstrable causes are constantly, and every where present to produce all the affections attributed to this far-fetched power; causes which are highly competent to the end, and supported by the most rigid analogies drawn from all that is yet known of the philosophy of medicine, and of animal nature. That sudden changes of particular habits; debauchery, after extreme abstinence; violent exertions after long fasting; sudden exposures to a hot sun, and other vicissitudes of weather; specific contagions,

such as syphilis, small-pox, &c. can, and actually do excite a multiplicity of disorders; and that too under circumstances unaccompanied with the slightest evidence of an electrical influence, I think are positions, so universally received, as to require nothing farther in this place in their behalf. If what I have advanced be true, I should therefore conclude that, any necessity for an invisible, gratuitous, and supposititious something to aid us in our enquiries, must be wholly superceded by more intelligible powers.

About the year 1800, and subsequently to the promulgation of the foregoing opinion, Noah Webster, of this country, published an elaborate treatise, written for the express purpose of showing that all epidemic diseases could only be accounted for by some unknown operation of electricity. And after much research, thought he had placed the point beyond all farther controversy. He contended that meteors, volcanic eruptions, earthquakes, &c. are unequivocal signs of a disturbance in the electric fluid, which he held to be extended throughout space, and to constitute what Newton called ether. That during such disturbance, this fluid, as a natural consequence, becomes unequally distributed in the atmosphere; being deficient in some portions and excessive in others, thereby either corrupting or changing the constitution of the air, or directly affecting the nervous sympathies of the body, lays the foundation of those diseases. He presumed that they might be divided into two classes of high, and of low action, depending upon an excess or deficiency of this hidden principle. As it regards the primary and immediate action of the electric agent, his notions do not essentially differ from the preceding, except that while he confines it entirely to epidemic diseases, the Abbe em-

braces under his, every malady to which the human system is liable. But when we come to consider the results of concomitant investigations, with which his doctrine seems necessarily connected, or rather upon which it essentially rests, it appears to me tenfold more pregnant with assumptive data, false arrangement, and absurd inference, than that of Bertholon's. For here we are required to wade through a series of the most perfect assumptions in order to arrive at his main principle; which last has nothing, either of an extrinsic or intrinsic nature to support it, except what it derives from the data so assumed. He supposes,

1. The immensity of space to be filled with the electric fluid, as a necessary atmosphere for the propagation of light through the distant parts of the planetary domain.

2. That this fluid is the soul of the material world, whereby all impulses, connected with natural events, are made.

3. That all violent convulsions in nature, such as tornadoes, earthquakes, volcanic eruptions, excessive seasons, epidemic mortality among the brute, as well as human creation, are concomitant and immediate effects of a disturbance in the ordinary distribution of this fluid.

4. That comets, meteors, planetary aspects, &c. *may* influence this distribution, after some *certain unknown* manner.

5. That the human body, in common with every other part of the terrestrial creation, may become charged with an excess or deficiency of this important principle.

6. And lastly, that this excess or deficiency actually lays the ground-work of all epidemical affections, by some how disordering the nervous, and other susceptibilities of the system.

In addition to the difficulties and objections, which I have already mentioned as necessarily lying in the way of the doctrine, under any view of its features, I would here remark,

1. That to give any respectable force to his ideas, every principle of common sense demanded of Mr. Webster, some proof, either from experimental result, or from rational demonstration, of the truth of his suppositions.

2. It was incumbent on him to show that no other causes existed, to which the phenomena might, with equal propriety, be attributed; which has not been done, except by mere surmise.

3. He should, at least, have rendered it plausible, that earthquakes, long droughts, &c. might not be produced by other agents, already admitted on as high philosophical credibility, as that of the electric fluid, in giving rise to disease.

4. I discern no cogent reason for the limitation he has given to the action of this agent, by confining it to particular maladies, while death equally ensues from other admitted causes. Upon the face of the universality, and immediate importance of this fluid in every natural event, it becomes difficult to conceive why it should so exert itself upon animal nature, as to produce epidemical disorders, and no other. For we do know, that during the prevalence of those very epidemics, a thousand other affections occur of an entire different type; and if symptoms be the criteria, not at all referable to electricity, in any manner whatever.

5. Admitting that either a plus or a minus state of the electric fluid be adequate to the production of those diseases, the theory would still be extremely defective, unless we were informed of some demonstrable criterion, whereby this super-abundancy, or deficiency might be determined at any par-

ticular period to exist. So far as I am acquainted with the phenomena of natural electricity, as well as with authentic accounts of pestilence at large, I feel warranted in affirming, that it has prevailed over and over, not only in all its forms and stages of malignity; but also under every variety and modification of appearance, presented either by the natural or artificial efforts of the electrical fluid. It appears to me therefore, clearly manifest, that Mr. Webster's notions rest upon speculations too far-fetched and conjectural to entitle them to professional confidence.

Dr. Shecut of Charleston, is the last individual, who has expressly advocated the opinion in question. His notions being fundamentally identical with those of the gentlemen already adverted to, a few words may suffice for his case. I feel a pleasure in doing his ingenuity the justice to say, that, however erroneous the hypothesis in itself may hereafter be demonstrated to be, he certainly deserves all the credit which may be due for having reduced it to some more intelligible form than either of his predecessors have done. Confining the agency of electricity entirely to epidemics, he has advanced so far as to specify the particular disorders which depend upon an accumulation of the electric fluid, as also those which arise from its deficiency. He then speaks of what he terms an electrical equilibrium, which supposes some medium natural quantity. It is further contended that, while this equilibrium exists, the door is closed against the prevalence of epidemics. The natural signs of a destruction of this healthy or innocent state of the fluid, consist in much thunder and lightning, or their total absence, &c. This powerful agent is presumed to effect its purposes upon the animal economy, ultimately, by some how gene-

rating a gaseous poison in the atmosphere. Which poison is supposed to disorder the healthy energies of the system, either through the function of respiration, or by a direct action upon the olfactory nerves, and in that way produces disease.

In addition to what has already been preferred against the general agency of electricity in deranging the functions of animated nature, I have little to offer as to Dr. Shecut, except a few remarks founded upon some trivial modification arising out of his specific details. In addition to the unsupported conjectures, and erroneous inferences which are essentially interwoven throughout the tissue of the hypotheses of Bertholon and Webster, the Doctor, as if aware of their fallacy, has attempted to furnish them some support by simply connecting one more link to the chain. Instead of stopping where those gentlemen did, in referring all the effect to the direct and immediate action of the electrical fluid, he conceives it better able to effect its purpose through some intermediate power. A defect which is enormous, as well as obvious upon the most superficial view of such an idea, is, a total absence of all explanation of what this gaseous poison is supposed to consist. Certainly, if it be a real existence, it must exist after some manner; it must have some mode or condition of being, and is therefore tangible or demonstrable. If it be demonstrable, it must be simple or compound, and is therefore the result of some specific process. He somewhere vaguely speaks of a possible super-oxygenation of the air, as being the probable mode of the operation of electricity, in exciting certain affections. But this has not only not been proved to be the fact, but is directly rebutted by the most cautious results of eudiometric chemistry, as also by every experimental demonstration of the philosophy of its action upon the

constitution of the air itself. On the highest mountains, and in the lowest vallies, during every season and condition of the year, its constitution, and the proportion of its proper ingredients, are invariably the same. Nor is there any tendency to an excessive state of the oxygenous portion of the air, ever discoverable by the action of electricity upon that fluid during any of its artificial applications. He also, in what he offers as aphorisms on the term miasmata, speaks of a variety of specific gaseous poisons; some are entirely vegetable, some animal; from which he deduces a hydrogenous poison, an azotic poison, an amoniacal poison, &c. together with various combinations of these. But when we come to make any analysis of their substance, or to institute any comparison of their import with the true philosophy of decomposition by putrefaction, as contained in our best and latest chemical authorities, their perfect vagueness, as well as repugnance to experimental result is so very striking, as to set aside the necessity of any farther refutation. I would therefore ask, what it is that constitutes this gaseous poison which gives rise to yellow fever? Does it consist of the electric fluid united to oxygen, or with nitrogen, or with the air itself? Does any one gaseous product of decomposition, or all of them together, united to electricity, form it? Nothing of the kind is explained, or enforced in a manner at all intelligible.

I have deemed it proper to say thus much in refutation of this entirely hypothetical and unsupported doctrine, by reason of the celebrity of its original promulgators, as also of the acknowledged learning and abilities of certain modern commentators and reviewers, who have thought it worthy of serious consideration.

SECTION II.

THE DOCTRINE OF CONTAGION.

The advocates of the doctrine of contagion, as applied to yellow fever, contend for the existence of some specific morbid matter, generated in the human body, and most generally accompanying human or marsh effluvia; which when taken into the body, some how reproduces itself, and by emanation, produces the disease in others. It is thus said to propagate itself by contagion or infection without any extrinsic aid. That this matter may become fixed in blankets, old clothes, packages of goods, &c. and being thus conveyed to distant places and there exposed, is capable of exciting the disease in as high malignity, as at its original seat. This is an abstract and declarative expression of their real tenets. Indeed, were we to credit all the stories which are told us of those instances of foreign exposure, by many of the less qualified believers in this fanciful principle, we should be constrained to palm upon it a kind of self-accumulative faculty during its passage or confinement, which enables it to display its energies with renovated and redoubled fury, when again disseminated. But on a closer examination, we discover an important feature in their system, which, though pretended by some to be unnecessary, is yet acknowledged by all to facilitate the operation of their contagious particles, in so remarkable a manner, as in my opinion, is sufficient to render the credibility of the whole very dubious at first sight.

They all agree, that yellow fever is not contagious, except under particular circumstances; an assertion, which if true, would destroy one of the best tested distinctions that was ever made in any science, viz. the distinction between contagious and non-

contagious maladies. Will any one be so bold as to assert that small-pox, measles, kine-pox, psora, syphilis, &c. require one situation or period in preference to another before they can act? They require *contact, or unequivocal exposure*, and do they ever fail to take effect when these conditions have existed? or does the one ever loose its pathognomonic signs by being absorbed into any of the rest? I presume no such thing has ever been witnessed. But what are the circumstances which are confessedly necessary to render the disease in question contagious? Upon minute inquiry, we find them to be highly hot, moist seasons, and places of most confinement, and where most putrefaction of every kind is constantly going on; or, as I should say, wherever all the causes usually assigned, on the score of heat, moisture, putrefaction and confinement, prevail in a high intensity, there yellow fever may exist; and where none of these causes are to be found, there it assumes the aspect of a mild bilious affection. Can gentlemen be serious when they express themselves thus? Again, how often does it prove *non-contagious*, compared to its *apparent* occurrences in the opposite form? I answer, about ten thousand to unity. I allude here to histories which cannot be disputed on any grounds; and not to those perverted, distorted, exaggerated, and in many instances, almost fabricated cases, which I regret to believe have too often been resorted to in our profession to support a favorite hypothesis, or to heal the wounds made, by rigid demonstration, in the falling pride of deluded imagination. It is said, these ten thousand require to be brought into particular habits of life, states of the body, &c. before those particles, which were so independent, desparate and fastidious in the one case, can exert their envenomed acrimony. Without

going a step farther, it must be obvious to the weakest capacity, that there is not a malady to which humanity is liable, which may not by such reasoning, be asserted with safety to be contagious. If two persons in the same family be afflicted about the same time, with hepatitis, we have only to declare, that the one necessarily caught his complaint from the other; if but one have the disease, we then say the circumstances are absent, which might render it active in the other; and I ask in either case, who could ever disprove our assertions? Common sense might inform all parties, that debauchery, intemperance, exposure, &c. had occasioned the affliction in both; and this I apprehend would be the fact, totally unconnected with any common principle.

If there exist certain genuine, contagious atoms, which give rise to yellow fever, I would respectfully ask, what is the nature of the change which they must be presumed to undergo in passing to, or assuming the harmless state? What natural agents are concerned in effecting the conversion, or in originally stamping upon them their properties? They are something, or they are nothing. If the latter, then they form not a subject for philosophic investigation; if the former, then, as has been said of Dr. Sheeut's gaseous poison, they must be made of some kind of matter, and are therefore solid, extended bodies; and if solid and extended, it certainly behoves the advocates of their agency to offer some other proof of the fact, than the bare circumstance that they are unable to explain the effect without them. No man of common sense, much less an accomplished logician, would coincide with me in saying, that because a phenomenon exists, and I cannot explain it upon the commonly received data, I am therefore at liberty to assert

what I please about it. We perceive every night, a number of shining bodies in the starry firmament, called fixed stars; no man has yet demonstrated their nature, texture, composition, or uses; nevertheless, I cannot feel myself at liberty to pronounce them so many actual gas lights, designed to illuminate the distant skirts of the universe. How far this is the predicament of the advocates for the contagious nature of yellow fever, I leave to the candid inquirer to determine. I would observe, that a majority of the physicians of the present day stand strenuously opposed to the hypothesis, whilst it has not been adopted to its fullest extent in former times, but by a minority of those who had seen most of the disease, and who were best qualified by previous acquirement to trace effects to their appropriate causes. And it will also be remarked, that there is not to be found a writer on the subject whose experience was respectable, who has not somewhere spoken of cases which were confessedly inexplicable upon the doctrine of contagion; or expressed a belief, that to render the disease characteristically contagious, other powerful extrinsic causes were invariably requisite.

I think I should be safe in asserting, that in all those cases where the disease has seemed to propagate from one person to another, were we to examine the history of this latter, it will be found to have been amply exposed to all the causes, which are known to be competent to produce the highest grades of bilious fever to which yellow fever is universally admitted to bear the most striking similitudes. I regret that those who have taken it upon them to record histories of the malady, have not at all times been as minute as we could desire; but I think, on a fair investigation, there will be found the most satisfactory proof of the correctness of the

above position; although much allowance be made, in relation to the cases we have handed to us, for the distortion of facts, and suppression of circumstances, by gentlemen in their advocation of favorite sentiments with the vindication of which they have falsely conceived their judgment and reputation concerned.

In addition to the foregoing, I may assume it as a truth, supportable upon the highest authority, that in every instance when a person laboring under yellow fever, has been removed to an elevated part of the country where none of the generally supposed causes operate in a high intensity, the symptoms have all ceased with the individual patient, without in any way having affected either physicians, visitors or attendants: facts which are incontrovertibly established in my estimation by the best authorities; and which are totally irreconcileable with any such idea as that of specific contagion. I will not encroach farther upon the intended limits of this essay by quoting cases in refutation of this doctrine; it having been already done in such an able manner by Caldwell, Rush, Bancroft and others, as to render such an attempt on my part perfectly presumptuous. I feel constrained however to remark, that but for the writings of one man, I cannot think there would have been found a liberal physician at this day, in the belief of such a principle. And by comparing his statements with those of other men, whose talents and opportunities were at least equal to his own, we discern much evidence, either of a destitution of candor, or that his understanding was extremely liable to be bewildered and his judgment led astray by appearances, which it were unpardonable not to have penetrated, especially when about to found upon them a theory which involved so many important

consequences to the philosophy of medicine, as well as to the usages and interests of society at large.

CHAPTER III.

THE DOCTRINE OF THE DIRECT INFLUENCE OF MIASMATA.

THIS opinion consists in attributing yellow fever to a *directly* deleterious operation of marsh effluvia, and other products of putrefaction, on the system. It is upon the overthrow of this notion, that the validity of my own must ultimately rest; for such is the nature of the facts adopted in each, that if one be true, the other is necessarily false. It is important here, that we distinguish between power and cause. For on this distinction rests, not only our ability to embrace all the circumstances concerned; but also much of the force and efficiency of our reasoning from the data so embraced. By a cause, to which an effect is referable, can only be understood the sum total of power concerned in its production. But such is the physical organization of material nature, that many powers may combine their tendencies to a particular end, while neither, separately, would be adequate to its accomplishment. Now, as no one of these powers, but the whole together, constitute the cause, common sense would tell us, that in explaining the effect, we should include the whole, in order that we might assign to each its appropriate character and influence. Indeed to select one, to the exclusion of the rest, would be to take such a partial view of the matter, as is strenuously forbidden by every sound principle of induction.

That marsh effluvia, or the gaseous exhalations, which arise from vegetable and animal matters during putrefaction, under a hot sun, do exert an influence in the production of yellow fever, and that to a considerable extent, I would not pretend to

deny. Indeed, I think their agency almost necessary to that effect. But I shall attempt to show, that to the doctrine as it is advanced, the following objections stand demonstrably opposed, viz.

1. That it adopts only *one* of a number of powers, each of which is equal, if not superior to itself, and all alike essential to the process.

2. That in the explanation of its mode of action, principles have been resorted to, which are directly contradicted by observation and experiment.

I will now proceed to substantiate these objections on the double ground of reason and experience. The first may be referred to the next chapter, in which will be found, a specific exposition of all the powers which I presume to be concerned in the excitation, or rather in laying the foundation of the disease. The second will be the subject of the present chapter, viz. That, admitting a considerable extent to the operation of marsh and other miasmata, the common illustration of that process, is so much opposed to the clearest results of observation and experiment, as to entitle the theory to the appellation of a mere hypothesis at best, and nothing more.

No one disputes that, without a constant supply of oxygen gas, or atmospherical air, plants, as well as animals, invariably die in a very short time. It is also true, that when the processes of vegetation, and animal respiration, are conducted in closed vessels, the oxygen disappears, the subject dies, and a product is obtained which is incapable of supporting any life whatever; for all living matter immediately dies when immersed into it. This product is a gaseous body termed carbonic acid. But will it be said here, that the death of the beings submitted to experiment, are to be attributed, in any manner, to this product, much less to a directly deleterious operation? I presume not. If a substance is pro-

ved to be absolutely, and exclusively necessary to a particular process, is it not a most natural inference that, if that substance be withheld, the process must cease? Is it at all necessary to suppose any product of the process, requisite to effect its cessation? I fancy not. It seems then to be essential to vitality, that, so far as atmospherical air is concerned, its oxygen be consumed, and carbonic acid be a result, merely springing out of such consumption. What then is the source of death, or the cessation of the living process? Most evidently, the absence, or conversion of that important pabulum of life, the oxygen; whose abstraction, upon the principle just laid down, is highly adequate to that end, without a requisition to refer any part of the effect to the carbonic acid. The logical bearing of these remarks, which are founded upon the nicest experiments, should be well attended to; for in that we shall be enabled to distinguish between a true and a false cause; between a necessary and an extrinsic power, in the production of effects: A distinction which is of invaluable moment in every investigation, where the connexion between effects and their causes are at all complicated; or where actual appearances are contravened to negative operations.

Let us now inquire what those noxious gases are, to whose action so much evil has been attributed; and whose origin consists in the decomposition of animal and vegetable matters by putrefaction. In this research I know of no source which is likely to afford any clue to our object, but chemical analysis; the philosophy of that science, whose energies embrace all physical truth, and whose practical applications to the comforts and ornaments of man, from their extent and diversity, have already given it an ascendency over every other topic of human intellection. We will commence this enquiry by sta-

ting what material bodies and circumstances are absolutely essential to every putrefactive process of the kind, to which I am alluding: These are,

1. Animal, or vegetable matter, or both.

2. Moisture, or water, either belonging to those substances themselves, or from extrinsive sources.

3. Atmospherical air.

4. Heat; and during yellow fever seasons, in a degree ranging from 80° to 95°, most generally. Without which requisites putrefaction neither commences, nor continues. The natural and chemical constitution of the above materials, as ascertained by specific investigations, are as follow, viz.

1. Animal matter affords, by analysis, four bodies, whose simplicity entitles them to the appellation of elements; for no experimental measures, hitherto instituted, have been adequate to their decomposition, or to render them more simple. These are carbon, hydrogen, nitrogen, and oxygen.

2. Vegetables are composed of carbon, hydrogen, and oxygen; and some contain a little nitrogen; as furnished by the same processes, as the above.

3. Water is a compound of oxgyen and hydrogen.

4. Atmospherical air is formed of oxygen and nitrogen.

These, with heat, being elements, are the only ingredients, ever alledged to be concerned in the formation of those effluvia, to which the disease has been attributed. They, therefore, either in a free state, or somehow newly combined, are the only products which we can legitimately anticipate to be found in the atmosphere; and such, upon observation and analogy, may be well demonstrated to be the case.

At this stage of our inquiry, it may be asked, by what kind of agency is an entirely new set of

compounds formed from the before mentioned original materials, and what is its manner of action? An answer to this question, embracing a complete experimental and philosophical exposition of its subject, would require a digression into the science of chemistry, incompatible with the intended brevity of this treatise; and, as I think, unnecessary to my purpose. Common sense, however, would suggest to any one, ordinarily versed in the phenomena of chemical action, that before any new combinations could take place among the substances properly within the sphere of action prescribed, the affinities, by which the original compounds were held together, must be broken up. This may be effected, either by the direct agency of heat; by what is termed disposing affinity; or by both conjointly. The initial effect of a physical exertion of heat upon bodies, is a separation of their particles, by which their attraction, for each other, is proportionately diminished. If at the moment of such separation, there be present a set of particles of a different nature, the two sets may unite and form new products, which, but for this agency of heat, might never have existed. The propriety of the term *disposing affinity* rests on the fact, that two bodies will often refuse to unite, unless one of them be previously united to some third body. So that in the case before us, a species of chemical energy may be thus excited into action, and form binary, ternary, &c. compounds; which energy might never have been brought into play, but for the presence of some third product, also, disengaged at the same moment. These remarks will facilitate a conception of the following rationale of the formation of the various products of putrefaction, under the conditions prescribed in a former paragraph. Though not professed to be complete, it is sufficiently sup-

ported by actual experiment, as also by many propositions, which are analogically established, respecting the laws which the gases in a nascent state, observe in their action upon each other, to meet the intention with which it is here drawn up. We have, then, only four simple elements, viz.

1. Oxygen of the air—of the water—of animal, and of vegetable substances.

2. Hydrogen of the water—of animals—and of vegetables.

3. Nitrogen of all animal; of some vegetable matters, and of atmospheric air.

4. Carbon of all animal and vegetable bodies.

Now, if we suppose all those elements to begin, to be disengaged, simultaneously, we may easily conceive that a portion of carbon may unite to a portion of oxygen, and form *carbonic oxyde gas.* That another portion of carbon may receive an additional dose of oxygen, and form *carbonic acid gas.* A third portion of carbon may, by its affinity for hydrogen in a nascent state, form with this body, *carbureted hydrogen gas.* A portion of nitrogen may unite to another portion of hydrogen, and form *amoniacal gas.* Another portion of nitrogen gas, by its affinity for oxygen, may combine with a portion of this, to form the *nitrous oxyde gas.* And as most animal, and some vegetable substances, contain sulphur, another portion of hydrogen, during its nascent state, may dissolve this sulphur, and thus constitute *sulphureted hydrogen gas.* There may, also, spring from this complicated play of affinities, other products, as water, nitric acid, nitrous gas, &c. but as they, either at their formation, or very soon after, assume the solid state, they may be thrown out of the present inquiry. We have, therefore, as permanently elastic aeriform bodies, products of putrefaction, the following list

of gases, which are the only substances we are at all warranted in supposing to exert any influence in giving rise to the effects attributed to marsh, and other effluvia, viz.

1. Carbonic oxyde gas. 2. Carbonic acid gas.
3. Carbureted hydrogen gas.
4. Amoniacal gas. 5. Nitrous oxyde gas.
6 Sulphureted hydrogen gas.

To these then, and to these alone, including atmospheric air, must our attention be directed in any investigation of the theory under consideration. If what has now been said, be admitted, a question naturally presents, viz. which one, two, or more of the above gases, constitute the principal agent in producing the effect; or shall we refer it to some conjoint action of the whole? If speculators will insist on causes, it is certainly their duty to assign them some shape or limit. Without some such definition, we never know what view to adopt of their *modus operandi.* It is asserted that they exert some direct action upon the system; but how far the assertion is warrantable, will be discerned in the course of this essay.

Experiment has determined, that of our list of noxious exhalations, or rather of the products of animal and vegetable decomposition by putrefaction, some are respirable; others non-respirable.—Of those again, which are respirable, some support life in a small degree, others not at all. Moreover, the effects of any, or all of those may be taken in a two-fold point of view; in a concentrated, and in a diluted state, during their action. In order that the subject may not be embarrassed by confounding too many views at once, I will distribute the remainder of what I have to advance against this hypothesis, into the four following sections, viz.

1. The effects of pure atmospherical air on the animal economy, together with the true philosophy of its action.

2. The effects of those gases, which are unfit for life, when breathed in their pure state.

3. Their effects when respired, in the diluted state in which they exist, as supposed causes of yellow-fever.

4. Deductions warranted by the foregoing sections.

SECTION I.

PURE ATMOSPHERIC AIR.

It is now generally admitted that during the germination of seeds, the vegetation of plants, and the respiration of animals, a most important change is necessarily effected in the constitution and properties of the air which is breathed, or brought into contact with them. Many very exact, and ingenious experiments, appertaining to chemical physiology, have proved that this change is identically the same in each, viz. The disappearance of the oxigen, and the formation of a corresponding proportion of carbonic acid, as has been already observed. This certainly exhibits an astonishing coincidence of action and result, when considered as belonging to three orders of animated functions, differing so widely in their immediate organization, and states of being. In addition to this circumstance, the following phenomena seem invariably to accompany animal respiration in the more perfect, red-blooded species, viz. the invigoration of the subject, and the conversion of the dark colored blood, as it passes through the lungs from the extremities of the system, into that of a bright scarlet, to be again distributed as before. Upon a deliberate view of the above facts, it will be perceived that the following circumstances, as connected with the operation of air in its subserviency to the purposes of animal life, require explanation, viz.

1. The disappearance of atmospherical oxygen.
2. The formation of carbonic acid.
3. The origin of the carbon.
4. The dark color of venous blood.
5. The bright scarlet of arterial blood.
6. The necessity for the change, and,
7. The manner in which it is effected.

The following considerations may lead to a just conception of the whole business:

1. The blood, as it goes to the lungs, bears a dark, grumous aspect.

2. The air which is inhaled into the lungs, at the same instant, loses its oxygenous portion, and receives, in its stead, nearly an equal bulk of carbonic acid, which, with some aqueous vapor, and the original nitrogen of the air, is expelled at the next exhalation.

3. At the self-same moment the blood becomes of a bright scarlet, which it again loses during the circulation, whence it returns to the dark color above-mentioned.

4. If this process be stopped, the animal immediately dies. These are topics of high importance to the philosophy of this wonderful function of animal respiration; and upon which much talent has been exercised, much laborious research instituted, and many ingenious speculations masterly conducted. They all, however, with their respective modifications, resolve themselves into this alternative, viz. either, that oxygen gas actually enters the circulation, and mixing with the blood, some how forms carbonic acid during the rout, which is disengaged by the lungs; or that this oxygen never goes into the system at all, but merely abstracts carbon from the blood as it passes through the lungs, and that the formation of the carbonic acid there takes place, and no where else. To state it more explicitly in the form of a query; does respiration support life by furnishing an inlet to some necessary principle, *oxygen*, from without; or does it accomplish that important end by facilitating the outlet of some surplus matter, *carbon*, which, if suffered to accumulate in the system, might destroy life? This is fairly the question now in point. My rea-

sons for adopting the latter member of the above alternative, may be drawn from the following propositions, whose correctness is founded upon actual experiment and calculation.

1. The carbon of the carbonic acid did not exist in the air inspired, and therefore must have been derived from the blood while it passed through the lungs.

2. By a precise analysis of the carbonic acid produced, we obtain every particle of the oxygen which essentially entered into the composition of the air breathed.

3. Every particle of the atmospherical nitrogen returns unchanged either in quantity or quality, except what arises from the carbonic acid, and aqueous vapor before spoken of.

4. Neither loose oxygen, nitrogen, carbonic acid, nor atmospherical [redacted] has ever been detected in air the blood, or other circulating fluids of the body, by any method hitherto tried. To explain, therefore, all the phenomena appertaining to the process, it is unnecessary to suppose oxygen, or any thing else to enter the system through the function of respiration. To the above propositions I will add three others, which at this day are admitted as fundamental, by every chemical physiologist in their speculations relative to this subject: these are,

1. That an ordinary man discharges by the lungs, about eleven ounces of pure carbon, every twenty-four hours.

2. By no other means at present existing in nature, could the elimination of this carbon be effected, but by atmospherical oxygen.

3. That the most minute portion of any foreign matter injected directly into the venous system of an animal, immediately destroys life. These are results, which rest, not upon speculative ingenuity,

but upon calculations and experiments too purely mathematical to admit of a doubt. From all of which it may be legitimately concluded:

1. That the natural color of blood is a bright scarlet, and being the appropriate food of the several organs of the body, is distributed by the arteries to the parts destined to become nourished. Its elements there separating and combining with those parts to form bone, muscle, nerve, blood-vessel, &c. there arises a surplus carbon. Hence the source of the carbon discharged by the lungs

2. If this carbon be suffered, or forced to accumulate in the system, it would soon destroy it, as so much foreign matter. Hence the necessity for its constant discharge at every moment of its formation; and to effect which the human head is not adequate to conceive so appropriate an apparatus, as that of the lungs and atmospherical air.

3. The manner of the disappearance of oxygen, the discoloration of the venous blood, and the formation of carbonic acid, consist simply, in the play of some powerful affinity between the oxygen of of the air, and the loose carbon of that fluid.

4. If venous blood contains carbon, and is black, and upon discharging this carbon, becomes red, it is very rational to suppose that there exists, at least, some necessary connexion between that blackness and the carbon; even though this latter may not be demonstrated to be the *direct* cause of it. If we put a few drops of ink into a tumbler of water, the whole assumes a dark color. Now in this case, we would not only attribute the color to the ink, but we would naturally conjecture, that upon withdrawing the ink, the water would return to its original limpidness. If a vegetable blue be reddened by an acid, we not only account for the redness by the direct agency of the acid, but we suppose that

if this latter by any means be abstracted, the blue color will naturally return. And such is actually the fact. The application of these remarks to the changes of color, which are constantly going on in the blood, is too obvious to be farther insisted on, without extending this essay beyond the compass it was intended to embrace.

SECTION II.

GASES WHICH ARE UNFIT FOR LIFE, IN A PURE STATE.

The subjects of this class of aeriform bodies are divisible into two orders, viz. those which cannot be respired at all, that is, they cannot obtain admittance into the lungs; and those, which, though they readily enter the lungs, yet afford no support to life whatever. These I will treat in the order mentioned.

1. The *non-respirable*. The only ones of this order, to be found in our list, are the carbonic acid, and amoniacal gases. If then, they never get into the lungs, how do they act in destroying life? A remark, almost universally made, in regard to their action, is, that they seem to close up the orifice of the windpipe, suddenly, by exciting a violent spasmodic contraction of the small muscles concerned in its structure. Indeed, from some experiments with re-agents, upon the liquors of the lungs after death in such cases, there can be but little doubt of this being the fact. We cannot, therefore, refer their effects to any peculiar inaptitude to support life, by producing the necessary changes in the blood, like atmospherical air, through a proper respiratory function. Upon the face of the facts, they cannot be said to have any association with such an office.

These gases are directed to, and spent upon, the *irritability of muscular fibre*, and of course have a relation, in their operation and effects, to that quality of the muscles of the larynx, and to nothing else. They are not in their chemical nature, calculated to extend their effects farther, nor are such effects ever observed. Neither the spasmodic contraction of so small a part of the body, nor any nervous impression as a consequence thereon, could be presumed to be adequate to the effect of death, *directly*. These gases, however, acting by their mass, their intensity, their quantity, stimulate the small muscles of the glottis, till they close the orifice, and this is all they can possibly do. But the closing of this orifice necessarily excludes the air upon which the continuance of life absolutely depends. And I do contend, that common sense forbids any other explanation of the matter. It is one which is really little more, or less, than a simple expression of the whole fact, and is therefore amply competent to our purpose. They take away life, negatively, through the medium of secondary causes, such as have been mentioned; which utterly banishes from our speculations and inquiries on this topic, every notion of putrefaction, &c. of the blood and other humors, together with all other such fantasies.

2. *The respirable.*—These are,

Carbonic oxyde.	Carbureted hydrogen.
Nitrous oxyde.	Sulphureted hydrogen.

These, as has been already suggested, again divide themselves into two kinds, under which it may be proper to treat them, viz.

1. Such as induce death gradually.

2. Those which are immediately followed by that catastrophe.

Of the first kind, the only one found in our list, is the nitrous oxyde, which contains about three times as much oxygen, per cent. by weight, as atmospherical air. Considering that oxygen is the essential principle in abstracting carbon from blood, it may seem somewhat embarrassing, that this gas should ever destroy life. But the mystery completely vanishes when we reflect, that death also ensues upon the use, even of oxygen, perfectly pure; that an immensity of effect often depends upon a disposing affinity, growing out of the proportion of principles brought within the sphere of action; that an agent, however natural and necessary in smaller quantities, may, and often does entirely defeat its purpose, in an over dose; and that the system, so generally under the control of habit, has been uninterruptedly accustomed to that specific combination of oxygen, which constitutes atmospherical air. Aqua fortis, nitrous oxyde and common air have precisely the same constitution as it regards the principles which form them; but the immense difference which exists between their respective properties, is entirely referable to the *proportion of the principles* by which they are constituted. Examples of this nature are numerous. No wonder then, that such an *unusual* substance, having such specific and permanent properties as nitrous oxyde has, should not subserve the purpose of respiration, in the more perfect animals. At all events, there is never discovered any change in itself, nor any new property in its products by respiration, that may not be readily accounted for, by the absorption of carbon from the blood by its oxygen, together with the action of the residual air of the lungs; of which more hereafter.

There remain now only three gases of our list, whose action in a pure state, is to be considered,

and whose use is followed by an immediate death of the animal respiring them; these are the carbureted hydrogen, sulphureted hydrogen and carbonic oxyde. They differ then, it seems, from the non-respirable gases, in passing into and out of the lungs, freely, without exciting the muscles of the glottis to contraction; and from the nitrous oxyde, in being followed by death instantaneously. I contend now upon what has been advanced as fundamental, on the highest chemical and physiological authorities, that we cannot explain their effects upon any other ground, than by an exclusively negative process, without doing violence to the first principles of ratiocination. If a constant supply of atmospherical air be essential to life; if these gases, as is obvious from their chemical characteristics, cannot supply its place; if, in no process of respiration, does any substance admitted into the lungs, ever get any farther into the system; and if, and it has never been pretended under the condition in point, no direct impression, or nervous excitement, from their mass or intensity, be adequate to produce death, the inference is irresistible, that death ensues by a negative circumstance, viz. the absence of pure atmospherical air. It certainly cannot be necessary to say more on this particular point. In relation to the nitrous oxyde and oxygen gas, in a state of purity, I would observe, that although any large accumulation of carbon in the blood, would inevitably destroy life, yet we are bound to presume the small quantity which is formed between each inspiration, to be somehow connected with the healthy functions or habits of that fluid, and therefore, if, as is highly probable with the two gases in question, it be abstracted too rapidly, the body may be equally deranged, or death actually ensue.

But it has been advanced that some of those gases do exert some direct influence upon the system, because of certain changes, which are said to havebeen observed in the color of the blood, after, or during the time they were breathed. To this position I object—

1. That many of the latest and most respectable experimenters have not observed these changes, although their attention had been directed expressly to this point.

2. That a variety of causes, growing out of particular states of the system at the time, imperfect apparatus permitting foreign matter, &c. might render such changes entirely accidental, and therefore no way related to the action of the gases themselves.

3. No proof exists, whatever, that any portion of those substances has ever been detected in the blood, and therefore if they acted at all, it has been in a way not yet pointed out. Again, it is urged that those bodies, by entering the lungs, have been diminished in bulk; which could only be explained upon the supposition, that the bulk lost, had been absorbed into the circulation. To this I would also reply—

1. That as to some of those gases, this diminution has never been observed at any time, by any one; and as for the rest, the circumstance is not at all uniform; which proves as much against the doctrine as for it, by stamping upon it, completely, the character of an accident.

2. Calculations founded on the most unequivocal admeasurement, aided by chemical experiment, have decided, that, after each ordinary expiration of an ordinary man, there is still a residuum in the lungs, of one hundred and nine cubic inches of aeriform matter, composed of some atmospherical air,

carbonic acid gas, nitrogen gas, and aqueous vapor. Upon which fact, provided this diminution had ever been satisfactorily established, no one at all versed in the phenomena and laws of pneumatic chemistry, could for a moment, be at a loss to account for it, upon principles, unconnected with any such idea as that of absorption.

SECTION III.

RESPIRATION OF THE NOXIOUS GASES IN A MIXED STATE.

A very few words on this topic will amply suffice. As to the non-respirable gases, I would ask, if they are presumed to act in this state, after the same manner, and upon the same principle, as was stated of them, when acting by their intensity or mass? To this question I should reply—

1. That, from their action in the diluted state in which they must be supposed to exist in the common atmosphere, no one has ever experienced any powerful nervous impression, nor has any one yet been suffocated by a spasm in the muscles of the wind-pipe; one of which circumstances, if not both, would be requisite, upon the assumption of a similarity of action.

2. Even admitting some *directly* deleterious consequences from their operation, when acting by their bulk or quantity, it does not necessarily follow, that an identity of action would attend their diluted or comminuted state. Innumerable instances may be adduced from the history of living action, under a variety of conditions, to show, that we are not warranted in laying it down as a physical law, that, because a substance in large quantities produces an effect, it will produce the same effect, or act in the same manner, in diminished portions. Arsenic, corrosive sublimate, tartar emetic, opium,

brandy, &c. in large quantities, are either violent poisons, or otherwise, actually induce death. Whereas the same articles, in lesser doses, produce the happiest effect upon the human system. So in the case before us, I might, with perfect impunity assert, that those gaseous exhalations of which we are now speaking, are rather intended to operate *some good*, against noxious causes during summer heats. And, although I might not be capable of demonstrating the fact, yet I am certain it would puzzle any one to directly disprove the assertion. But, I trust, the period is not far distant, when this rage for such totally unsupported speculations, will be viewed in our schools in its proper light.

3. If under the circumstances attending the prevalence of yellow fever, no such effects are ever witnessed as seem essential to the action of the non-respirable gases in their unmixed or concentrated state; if their action in this state affords us no clue to their real operations when suspended in our atmosphere; and if, as has been shown, under no conditions, can they be presumed to enter the system to corrupt its fluids, the inference is irresistible that they must act, if they act at all, after a manner not hitherto accounted for, or explained.

As it regards the action of the respirable gases, which support life in any degree, I hope enough has been said to prove the absurdity of accounting for their effects, upon any supposition, that has not an exclusive relationship to the abstraction or absence of oxygen, from the usual quantity of air, which is drawn into the lungs at each inspiration. How then do they operate in giving rise to the disease? We have seen that it cannot be by any direct impression upon the system, through the function of respiration. They cannot be presumed to accomplish their purpose by a direction of their

energies upon the olfactory nerves. There is no proof that they mix with the food or saliva, and thus act primarily on the stomach. Nor would it be a more plausible hypothesis to suppose them to be absorbed through the pores of the skin. It appears to me, that upon the commonly adopted notions, on this subject, there can be nothing, of an intelligible character, in any way affirmed of the origin of the disease. As soon as we attempt an analysis of its essence, the whole substance of the doctrine seems to vanish like a shadow. However let us proceed.

SECTION IV.

DEDUCTIONS FROM THE FOREGOING.

1. In no case do those effluvia termed miasmata, and to which yellow fever has been attributed, act at all, when breathed in their fullest intensity, except by their mass or bulk; which action relates entirely, either to some derangement in the irritative motions of particular muscles; to a disorder of the nervous sympathies of the body; or to a total exclusion of atmospherical air.

2. When breathed in the diluted state in which they necessarily exist, during yellow fever seasons, to refer their influence to a similar principle of action, except partially to the exclusion of vital air, would be to advance a supposition, which is unwarranted by mechanical, and contradicted by chemical philosophy; as well as destitute of the slightest sanction by any thing known respecting the agency of inanimate matter on the living economy of animals.

3. In those instances of supposed changes in the blood, or of alteration in the bulk of gases inhaled, we are to view them as perfect accidents, or at least, as wholly unconnected with any influence exerted directly on the system by the gases so employed.

4. When these noxious exhalations induce death, or yellow fever, I should deem it irregular, illogical, and therefore unphilosophical, upon the face of the facts and principles adverted to, to suppose the above consequences to have a relationship to any other of their properties, than their bulk in mechanically excluding the due quantity of the oxygen of the air which a healthy condition incessantly demands.

5. As it regards pure oxygen and nitrous oxyde, they support life for a time, only in consequence of the oxygen they contain; and death is to be accounted for upon the principle, first, of a want of the requisite adjustment among the bodies in action, whereby the proper disposing affinity is not established between the gas, and the matter to be abstracted from the blood; and, secondly, that an agent, however necessary in some proportion, may entirely defeat its purpose in an over dose.

6. As confirmatory in the highest degree, of all that has been advanced in opposition to the common doctrine of marsh and other effluvia, is the fact, that animals die as soon in actual vacuo, as in artificial atmospheres formed of those products, except the nitrous oxyde, whose particular action has been sufficiently illustrated. I have been thus minute in examining this third theory of modern sanction, because of all others, it is the most generally received, and because its refutation was an essential preliminary to the unfolding of my own views, which I shall now proceed to execute.

CHAPTER III.

The following constitutes an application of the foregoing principles and reasoning, to the particular circumstances of yellow fever, in which will be given a specific, but concise exposition of the several powers, which, by a conjoint mechanical effect upon the general atmospheric mass, lay the foundation of the disease; together with an explanation of the manner in which they are presumed to operate.

1. *The operation of heat.* I believe it will not be denied, that yellow fever never prevails, but at that period of the year when the sun's influence is at its greatest intensity, or thereabout. The rarifying agency of heat, on solid, liquid and gaseous substances, is too familiar to every one at all conversant with chemical investigations into their properties, to require any elaborate demonstration of it in this place. It will, therefore, suffice for my purpose, to give the result of some experiments which have been expressly instituted to enlighten this topic, as relating to aeriform substances. One hundred cubic inches of common air, heated from 32° to 212° of Fahrenheit, have their bulk increased to one hundred and thirty-seven and an half cubic inches. This being thirty-seven and an half for 180° by the rule of proportion, would furnish about ten per cent. for 45° the difference on an average, between the winter and summer temperatures of this climate. Here then is an obvious, intelligible cause, producing an absolute effect, that of enlarging the mass of the atmosphere without increasing its quantity. Now as one fifth of the bulk of one hundred cubic inches of the air is pure oxygen, it is evident, that if the whole be expanded to one

hundred and ten cubic inches, one hundred of the expanded mass will not contain the whole fifth of the air under experiment, that is, the whole of its oxygenous portion. Therefore, at no one inspiration of the expanded air of yellow fever seasons, do the lungs receive their due proportion of the vital gas. Life and healthy action having been demonstrated to depend, absolutely, upon a due and uninterrupted supply of this principle, the application of the power before us, to the existence of the disease in question, is too obvious to be farther insisted on.

2. *Aqueous vapor.* With but a few exceptions, and these very obscurely or equivocally stated, the prevalence of yellow fever is invariably preceded or accompanied by heavy or long continued rains. This water being immediately acted upon by a very hot sun, is quickly converted into vapor, which by chemical combination and mechanical suspension together, add vastly to the ordinary bulk of the atmosphere. I am unacquainted with any direct mode of ascertaining the absolute quantity of vapor, that would be produced by a given low temperature under a given atmospheric pressure in a given time. The celebrated Dr. Halley ascertained, that during a common summer's day, a circular surface of salt water of eight inches diameter, would lose by evaporation about six ounces. And according to the masterly experiments of Mr. Watts on the conversion of water into steam under the highest temperature, this fluid upon the exclusive principle of elasticity, will, in the latter state, occupy about eighteen hundred times its former bulk. But, as in the case before us, this high degree of temperature does not exist, the rising of the vapor is entirely referable to the two principles before-mentioned, viz. chemical union with the air in part,

and mechanical suspension by a difference of specific gravities. On this point, however, I shall rely on a general proposition, which was the result of a series of the most ingenious experiments to be found in the annals of analytic science, made by Mr. Leslie, on an instrument which he contrived as a photometer. Its use was founded on the principle, that bodies are always heated in proportion to their absorption of light; and by measuring the expansion, which gases underwent by the action of the heat so accumulated, he accomplished his object by a ready calculation. During his researches on this subject, he soon perceived the applicability of the instrument to purposes of hygrometry; and turning his attention more closely to its results in relation to the production of cold by evaporation, he came to the following proposition, viz. that water, at any ordinary temperature above 32° of Fahrenheit, evaporates in some direct ratio to the rarification of the air employed. Now what are the facts of the subject under consideration? We have the greatest quantity of water and moisture, together with the most powerful sun, and most highly rarified atmosphere to produce their vaporization. And, although I have not the requisite data for calculating the exact per cent. of enlargement, that the atmosphere would sustain under given circumstances of heat and moisture; yet, if what has been advanced, be admitted, (and I am sure no one can doubt its correctness) it must be sufficiently great to operate some important effect in the manner alluded to.

3. *Putrid animal and vegetable effluvia.* We have seen, that, two of the requisites to putrefaction, are heat and moisture. Its rapidity is always proportionate to the degree of temperature within the limit of 95° Fahrenheit; and the history of the

disease from all parties, informs us, that it never occurs till the heat of the weather has for some time ranged between 80° and 95°. If then, during those extremely hot, moist seasons, there should be present an unusual, or even an ordinary quantity of dead animal and vegetable matters, they must decompose, and speedily impregnate the air with their products. We also learn, that as it has occurred in divers places, it not only observes those periods of the year when there exists most heat and moisture, but it also obeys those circumstances of locality, which are most favorable to collections of such filth. Now what does chemical philosophy teach us relative to the conversion of solid and liquid bodies into the gaseous or aeriform state? I cannot at this moment, appeal to any direct and unequivocal experiments ever expressly instituted on the specific point before us. But we are amply taught by a host of facts connected with the condensation of the gases into solids and liquids, as well as, *vice versa* in part, and which are therefore perfectly relavent, that in the former they occupy many hundreds of times the space which they do in the latter. A cubic foot of oxygen and hydrogen gases in the proportion in which they form water, condense into a comparatively imperceptible mass. Nitrogen and hydrogen in the proportion to constitute amonia, are subject to the same law of diminution. The same remarks equally apply to many of the most familiar processes and experiments of the laboratory. Many cubic inches, or feet of hydrogen, emanate from a mixture of iron filings, sulphuric acid and water, without perceptibly altering its bulk. The same may be said of carbonic acid, disengaged from sulphuric acid and chalk; or of amoniacal gas, from muriate of amonia and quicklime. And in regard to the obtainment

of carbureted hydrogen from some species of pit-coal, the above observations are so strikingly verified, that, after the whole of the gaseous products are disengaged, their masses are absolutely increased in size; and when burned as fuel, afford more heat from a given weight, than the original coal if consumed in open grates. It cannot be difficult then to perceive, that if in yellow fever seasons, there should be much matter to be decomposed, the enlargement of the general atmospheric bulk, must be considerable as a necessary consequence.

4. *Vegetation of plants, &c.* From the circumstances already detailed, of the prevalence of yellow fever, it is evident that, it is necessarily preceded or accompanied by that period of the year when vegetation is most rapid, and consequently when most oxygen gas is consumed by it. It has been observed, that this principle is equally necessary to vegetable as to animal life; because if supplied with it, they afford the same products, and if debarred it, they inevitably perish. Though this, and the following source of the disease, do not act directly by increasing the bulk of the air, yet they certainly do, relatively to the quantity of oxygen in a given bulk, by a direct consumption of this principle, and a substitution in its stead of an equal bulk of carbonic acid. Its relevancy, is therefore, legitimately established.

I had intended to have detailed a few results of experiments, which have been instituted, relative to the quantity of oxygen gas converted under given circumstances, into carbonic acid. But upon examination, I discover such a want of uniformity of action, among different plants, differing in energy, in size, in number, &c. as presents obvious difficulties in the way of making any satisfactory calculations as to the absolute quantities throughout the globe. To cur-

tail this head then, I would merely observe, that it is now unequivocally determined, that all vegetables do absolutely consume the oxygenous principle of the atmosphere, precisely after the manner of the animal kingdom, affording as results, precisely the same changes in the chemical constitution of that fluid. It is also well agreed, that, most of the plants even of very small dimensions, will thus alter the properties of several cubic feet of atmospheric air in a very few days: and when we reflect on the immensity of subjects belonging to this kingdom of living nature, all over the earth; and that at the period when yellow fever is disposed to be prevalent, their action is most energetic, we cannot but attribute to their operation, great influence in the abstraction of the vital portion of the atmosphere. As to the restorative faculties, which have been attributed by some to the vegetation of such beings of every description, by which deteriorations of the atmosphere become neutralized, I cannot but view them as perfectly conjectural, or founded upon experiments wholly deceptive, and now proved to be erroneous. Farther illustration of this topic cannot be necessary, nor its application difficult. It is therefore submitted without farther comment.

5. *Animal respiration.* This point does not labor under the same uncertainty, as that which characterizes either of the preceding, and with some labor, there might be a very near approximation to the absolute consumption of the oxygenous ingredient of our atmosphere established. It has been well ascertained, that all animals affect the air, and require its agency in their sustenance, in some steady proportion to their size, or to that of their respiratory organs. These circumstances, with their number, being determined, a ready calculation might at once settle the actual results which are con-

stantly revolving in nature. I will therefore take the human subject as an example. A man of ordinary dimensions, will, on an average, inspire forty cubic inches of air at one time; and will repeat this twenty times in one minute, or twenty-eight thousand eight hundred in every twenty-four hours. Now of this whole quantity in bulk, one-fifth is pure vital air, which by conversion will constitute about forty-thousand cubic inches of carbonic acid, as a substitute. Then every day of his life, each man on the globe, consumes about forty-thousand cubic inches of pure oxygen; or corrupts and renders unfit to support life, two hundred thousand cubic inches of the common atmospheric mass. If we for a moment recur to the number of animals belonging to the brute and human creation, we cannot but perceive the immense extent to which they may influence the healthy properties of the air. It may be replied, that, these two last powers are in operation every year. But it is also true, that they do not at all times have the rest to unite their forces with theirs; and that when the others do occur, they all actually combine to the same end. The one, or the other, separately, might be inadequate to lay the foundation of the disease; but when acting in conjunction, might become competent efficient.

Let us pursue this application of all the powers which have been adverted to, as concerned in the production of yellow fever, to the above fact. It has before been stated, that one man is supposed to yield, by respiration, about eleven ounces of pure carbon in each twenty-four hours. This carbon, by its union with oxygen, in the proportion of seventy-two per cent. by weight, forms the carbonic acid before mentioned. We have also seen as a result of much ingenious and accurate research, that, the most minute quantity of foreign matter, of any kind, car-

not be injected directly into the venous system, without a risk of instantaneous death to the subject. There is then an absolute necessity for as much oxygen in the day, as will saturate eleven ounces of carbon, discharged from the blood. It must be plain, therefore, that if the system goes on generating carbon, and by any means, the forty cubic inches, which is the ordinary quantum for an ordinary man, at each inspiration, be deprived of its usual quantity of oxygen, this carbon must accumulate; and not finding an exit, would act precisely as so much alcohol, water, milk, &c. thrown directly into the veins. Again, should it be said, that the generation of carbon, by any law of the animal economy, diminishes with the diminution of the supply of oxygen, I would reply, that we must presume that function to be somehow connected with the healthy order of the body, and if it be suspended by a want of a due supply of the only principle, which is capable of keeping it up, the reasoning is still good, that the body must suffer a proportionate derangement.

To conclude, by an example illustrative of the joint result of all the circumstances, enumerated as the source of yellow fever: Suppose fifteen hundred cubic inches of atmospheric air to be employed in respiration. If a man inhale forty of these at each inspiration, and make twenty repetitions in one minute, he will of course consume the whole in about one minute and fifty-two seconds of time. Now the fifteen hundred, containing one-fifth of pure oxygen, gives three hundred of this latter, which is absolutely necessary to be supplied for each minute and fifty-two seconds. But if, by the expansive power of heat, the suspension of moisture, the exhalation of putrefactive products, the vegetation of plants, and the respiration of animals, this

fifteen hundred be so enlarged absolutely, and relatively to the quantity of oxygen, as to occupy twenty per cent. more of space, which would be eighteen hundred cubic inches, it must follow, that he would not obtain, in the same time, the whole three hundred of said oxygen. He can only inhale forty cubic inches at a time, and would therefore consume of the *expanded mass*, but fifteen hundred cubic inches in a minute and fifty-two seconds. But there would be still remaining three hundred of the expanded mass to make up the eighteen hundred; and this eighteen hundred contains, in all, no more oxygen than the original fifteen hundred did. If fifteen hundred contain one-fifth of oxygen, it is clear, that, if they be simply enlarged twenty per cent. the oxygen then constitutes one-sixth of their bulk. Now the one-sixth of the remaining three hundred cubic inches, of the expanded mass is fifty: which is pure oxygen. This deducted from the three hundred, which constituted the one-fifth of the original fifteen hundred of atmospheric air, leaves two hundred and fifty cubic inches as the absolute quantity of the vital principle, which a man, under the conditions prescribed, would consume in one minute and fifty-two seconds: Whereas previous to their expansion he obtained three hundred of oxygen in the same time. If there be any error in this kind of reasoning, I should be extremely happy to meet its exposition; but, as it at present offers itself, I feel a perfect conviction of its correctness, as well as of its relevancy to the cause of yellow fever.

I have founded the whole of my views and reasonings on the subject, upon an exclusive combination of physiological principles, with the results of chemical researches; having been long impressed with a belief, that, on this particular topic of medical

philosophy, no other known resources were at all likely to furnish any clue to a satisfactory, or even an intelligible conclusion. If we would disperse the veil, which at present hides from our veiw the secret energies of physical nature, and form to our minds a just conception of the recondite properties of matter; if we would govern our conduct, by an adherence to formal prescriptions of moral duties, founded upon correct ideas of the abstract attributes of the human intellect, by tracing the connexion which obtains between the intellectual faculties of man, and the properties of inanimate bodies; if we would exalt the truths, and appreciate the merits of revelation, by stripping the details of remote history of their anomalous features, and thereby confirm and improve our present notions of the attributes of deity, and the faculties of our own nature, together with the relationship we bear to our Maker, we must invariably attend to the phenomena and laws of chemical philosophy—of that transcendent department of science, which holds the keys of physics, of morals, and of divinity.

FINIS

John Lynam

One day after date